M. L'Hoste, a French aëronaut, crossed the Channel in a balloon on Sunday; he left the French coast at 5 p.m. on Sunday, and landed at Smeeth, near Ashford, at 11.

The additions to the Zoological Society's Gardens during the past week include a White-fronted Capuchin (*Cebus albifrons*) from South America, presented by Miss A. Tanner; two Common Marmosets (*Hapale jacchus*) from Brazil, presented by Mr. H. H. Forbes Eden; three Mexican Deer (*Cervus mexicanus* ♂ ♀ ♀) from the Island of Santa Cruz, presented by Capt. Edwin Cole; a Getulian Ground Squirrel (*Xerus getulus*) from Morocco, presented by Mr. Geo. D. Cowan; a Grey Ichneumon (*Herpestes griseus*) from India, presented by Capt. W. F. Small; a Common Squirrel (*Sciurus vulgaris*), British, presented by Master C. B. Webster; two Stink-pot Terrapins (*Aromochelys odorata*), a Pennsylvanian Mud Terrapin (*Cinosternon pennsylvanicum*), a Mississippi Alligator (*Alligator mississippiensis*), a Sharp-nosed Crocodile (*Crocodilus acutus*) from Florida, presented by Capt. E. Cole; a Common Chameleon (*Chamæleon vulgaris*) from North Africa, presented by Mr. F. L. B. Payne; a White-fronted Capuchin (*Cebus albifrons*), a Black-faced Spider Monkey (*Ateles ater*), a Pileated Jay (*Cyanocorax pileatus*), a Spotted Tinamou (*Nothura maculosa*) from South America, two Ruddy Finches (*Carpodacus erythrinus*) from Siberia, a Jackdaw (*Corvus monedula*), British, four Eyed Lizards (*Lacerta ocellata*), South European, purchased.

OUR ASTRONOMICAL COLUMN

The Total Solar Eclipse of May 6.—The *Comptes Rendus* of the sitting of the Paris Academy of Sciences on the 3rd inst. contain the reports from the observers sent by the French Government to Caroline Island in the Pacific for the observation of the recent total eclipse of the sun. The party was composed of M. Janssen, M. Trouvelot of the Observatory of Meudon, M. Pasteur, photographer, and an assistant, who were accompanied by Prof. Tacchini, director of the Observatory of the Collegio Romano, and Herr Palisa of the Observatory of Vienna, the discoverer of a large number of minor planets. One of the main objects of the expedition was a search for so-called intra-Mercurial planets, and it is to the observations made in this direction that we shall refer here. Herr Palisa and M. Trouvelot were especially occupied with this work. The former had a telescope of 6 inches aperture, with short focus and large field, equatorially mounted. M. Trouvelot had two telescopes, one of 3 inches aperture, with large field, reticule, and interior circle of position, and one of 6 inches aperture giving a high magnifying power. The 3-inch telescope formed a sweeping instrument with a field of about $4\frac{1}{2}$ degrees diameter, for the exploration of the circumsolar region. Both telescopes were on a parallactic mounting, and in order to secure rapid record of positions and dispense with the readings, which cause the loss of valuable time, M. Janssen had caused what he terms "tracelets de microscope" to be applied to the circles of right ascension and declination. Each of these, placed in the hands of an assistant, allowed of there being made, on the direction of the observer, a fine stroke across the divided circle and its vernier, so that subsequently, with the aid of this very precise indication, the instrument could be replaced in the position of the observation and the necessary readings made at leisure. It was arranged that MM. Palisa and Trouvelot should divide the work, each attending specially to one side of the sun. The Vienna astronomer's instrument, properly, as it seems, a comet-seeker, by Merz, had a magnifying power of 13, giving a field of 3°. With this, on totality taking place, he commenced his search, starting from the sun towards Saturn, at first on the south, and when he did not thus find stars he returned to the sun, and swept more to the north. In this way he recognised ***nine*** stars, all which are identified in the Bonn *Durchmusterung*. We give the list of stars, correcting two misprints in the *Comptes Rendus* (14°,355 should be 16°,355, and for 20°,542 we should read 20°,543), and appending the positions of the stars for the Bonn epoch 1855·0: thus, with the sun's place reduced to the same epoch, the relative positions of the stars with respect to his centre will be readily seen:—

Bonn Zone and number of star.	Magnitude.	Right Ascension.	Declination.
°		h. m. s.	°
16,355	5·7	2 41 13·1	+16 51·5
16,484	6·0	2 31 15·2	16 4·1
19,477	4·2	3 3 20·8	19 10·7
19,578	5·5	3 33 57·8	19 13·9
19,582	6·0	3 35 24·5	19 12·5
20,527	4.5	3 6 34·5	20 30·5
20,543	5·0	3 12 51.0	20 37·3
20,551	5·0	3 14 24·8	20 13·4
20,556	5·8	3 16 4·2	+20 17·4

The result of his search Herr Palisa states to be that, between the limits (1855·0) 2h. 52m. from +14° to +19°, to 3h. 40m. from +16° to +22°, there was no star of the fifth magnitude unmarked in his chart, this, it should be mentioned, being a lithographic chart supplied to him by Prof. Holden, one of the American observing party.

M. Trouvelot's attention was first directed to the study and figure of the corona, but, after the totality had lasted two minutes, he applied himself to explore the region west of the sun. He moved his telescope 10° in declination to the north of the sun's centre, and swept slowly from that point from east to west, to a distance of 15° in right ascension. The first sweep brought out a small whitish star; two other sweeps were made without any result; but in the fourth he saw a bright star of a decided red colour, which he estimated at 4 or $4\frac{1}{2}$ magnitude. Its approximate position was a little to the north, and a little to the west of the sun, but the cause of a more exact determination of position not being made will be best given in M. Trouvelot's own words:—"En voulant amener cet astre dans le champ très restreint de l'oculaire du 6 pouces (0·16m.), afin de chercher à constater s'il montrait traces soit d'un disque, soit d'une phase, il se produisit une certaine confusion parmi les deux aides que j'avais placés aux cercles horaire et de déclinaison pour guider la course des balayages, et bien que l'étoile traversât le champ visuel, il me fut impossible de retenir en place la lunette, et dès lors de reconnaître son caractère et sa position." In the abstract of results of observations appended to the reports of the observers, after reference to Herr Palisa's experiences, we read in the *Comptes Rendus*: "M. Trouvelot arrive à un résultat moins net pour le côté ouest, mais nous savons que cet observateur distingué désire revoir la région où se trouvait le soleil au moment de l'éclipse avant de ce prononcer." It is stated that the photographs, though not yet examined in a complete manner, appear to support the negative result obtained by Herr Palisa as to the existence of an intra-Mercurial planet.

A New Comet.—A Dun Echt circular (No. 78) notifies the discovery of a comet by Mr. Brooks on September 2, which was thus observed by Mr. Wendell at the Harvard College Observatory on the following night:—

	Greenwich M.T.	R.A.	Decl.
	h. m. s.	h. m s.	° ′ ″
September 3 at	16 9 24·5	16 35 15·6	+ 64 49 33

Daily motions in R.A. − 36s., in declination −12′. It is described as circular; less than 1′ diameter; tenth magnitude; well defined nucleus, and no tail.

THE GERMAN SURVEY OF THE NORTHERN HEAVENS[1]

The illustrious Argelander was accustomed to say in the quaint form of speech which he often employed, "The attainable is often not attained if the range of inquiry is extended too far." In no undertaking is there greater need of a judicious application of this sound maxim than in the systematic determination of the exact positions of all the stars in the visible heavens which fall within the reach of telescopes of moderate power.

The first subject which engaged the attention of the *Astronomische Gesellschaft*, at its formation in 1865, was the proposition to determine accurately the coordinates of all the stars in the northern heavens down to the ninth magnitude. To this association of astronomers (at first national, but since become largely international, in its character and organisation) belongs the credit

[1] An address delivered by Prof. William A. Rogers before the American Association for the Advancement of Science at Minneapolis on August 15, 1883.

of arranging a scheme of observations by which, through the co-operation of astronomers in different parts of the world, it has been possible to accomplish the most important piece of astronomical work of modern times. With a feasible plan of operations, undertaken with entire unity of purpose on the part of the observers to whom the several divisions of the labour were assigned, this great work is now approaching completion. While it is yet too early to speak with confidence concerning the definitive results which the discussion of all the observations is expected to show, we may with profit consider the object sought in the undertaking, the general plan of the work, the difficulties which have been encountered, and the probable bearing which the execution of the present work will have upon the solution of a problem concerning which we now know absolutely nothing with certainty,—a problem of which what we call universal gravitation is only one element, if, indeed, it be an element,—a problem which reaches farther than all others into the mysteries of the universe,—the motion of the solar and the sidereal systems in space.

Our first inquiry will be with respect to the condition of the question of stellar positions at the time when this proposal was made by the *Gesellschaft* in 1865. All the observations which had been made up to this time possess one of two distinct characteristics. A portion of them were made without direct reference to any assumed system of stellar coordinates as a base; but by far the larger part are differential in their character. This remark holds more especially with reference to right ascensions. Nearly all of the observations of the brighter stars made previous to about 1830 were referred to the origin from which stellar coordinates are reckoned by corresponding observations of the sun; but since that date it has been the custom to select a sufficient number of reference stars, symmetrically distributed both in right ascension and declination, and whose coordinates were supposed to be well known. The unequalled Pulkowa observations for the epoch 1845, form, I believe, the only exception to this statement. From the assumed system of primary stars are derived the clock errors and instrumental constants which are employed in the reduction of all the other stars observed. The positions of these secondary stars, therefore, partake of all the errors of the assumed fundamental system, in addition to the direct errors of observation.

The following list comprises the most important of the catalogues which have been independently formed: viz. Bessel's Bradley for 1755, the various catalogues of Maskelyne between 1766 and 1805, Gould's d'Agelet for 1783, Piazzi for 1800, Auwer's Cacciatore for 1805, Bessel for 1815, a few of the earlier catalogues of Pond, Brinkley for 1824, Bessel for 1825, Struve for 1825, Bessel for 1827, Struve for 1830, Argelander for 1830, and Pulkowa for 1845.

An analysis of these catalogues reveals four important facts:—

First, that, a large share of the observations relate to bright stars, at least to stars brighter than the eighth magnitude.

Second, that in a large number of cases the same star is found in different catalogues, but that no rule is discoverable in the selection.

Third, that, with the exception of the Polar catalogues of Fedorenko, Groombridge, Schwerd, and Carrington, the double-star observations of Struve, and the zone observations of Bessel and Argelander, the observations were not arranged with reference to the accomplishment of a definite object.

Fourth, that each catalogue involves a system of errors peculiar to the observers, to the character of the instruments employed, and to the system of primary stars selected, but that thus far there had been no attempt to reduce the results obtained by different observers to a homogeneous system. In estimating the value of these observations it will be necessary to refer to the researches which have been made subsequent to 1865.

The systematic deviations of different catalogues in right ascension *inter se* were noticed at an early date by several astronomers; but the first attempt to determine the law of these variations seems to have been made by Safford in a communication to the *Monthly Notices of the Royal Astronomical Society* in 1861 (xxi. 245), on the positions of the Radcliffe catalogue. I quote the equation derived by Safford, since it appears to be the first published account of a form of investigation almost exclusively followed since that time. It is as follows:—

Diff. of R.A. (Greenw. 12 Year Cat.—Rad.) = $-$ 0·38s. + 0·32s. sin (a + 5h. 32m.). Extending this expression to terms of the second order, it may be put under the form $\Delta = a$ constant + ($m \sin a + n \cos a$) + ($m' \sin 2a + n' \cos 2a$) +, &c.

Safford also seems to have been the first to notice the connection between the observed residuals, and the errors in position of the primary stars employed. He remarks, "In investigating the causes which would give rise to such systematic discrepancies, I was struck with the fact that the same or nearly the same variations were apparent in the assumed places of the time stars for the years since 1845; that, if the correct positions of the time stars had been assumed, the resulting positions would have been free from these small errors." That the relation given by Safford should have been observed at all is the more remarkable since the primary stars upon which the Radcliffe positions depend are nearly the same as those employed at Greenwich. In reality the systematic errors of both catalogues have since been found to be considerably greater than is here indicated, and the deviation pointed out by Safford is in the nature of a second difference. The speaker has shown (*Proc. Amer. Acad.* 1874, 182) that the weight of the errors of the provisional catalogue assumed fell between the first and the third quadrants in the Radcliffe observations for 1841–42, on account of the omission of certain clock stars which were used at Greenwich.

Since the discordances which exist between two catalogues may arise from errors in either one or in both, it is clearly impossible either to determine the nature of the errors or to assign their true cause until a fundamental system has been established which is free both from accidental and from periodic errors,—from accidental errors, since a few abnormal differences may easily invalidate the determination of the errors which are really periodic; from periodic errors, because a relative system can only become an absolute one when one of the elements of which it is composed becomes absolute.

We owe to the researches of Newcomb, published in 1869–70, a homogeneous system of stellar coordinates in right ascension, which are probably as nearly absolute in their character as it is possible to obtain from the data at present available. He determined the absolute right ascensions of thirty-two stars of the first, second, and third magnitudes, and comprised between the limits $-30°$ and $+46°$ declination. A comparison of the places of these stars for a given epoch with the same stars in any catalogue for the same epoch enables us to determine with considerable precision the system of errors inherent in that catalogue. Several circumstances prevent the exact determination of this relation. Among them may be mentioned the fact that Newcomb's system cannot safely be extended far beyond the limits in declination of the stars composing the system, that the stars are not symmetrically distributed in declination, and that the system of errors derived from bright stars is probably not the same as that derived from stars of less magnitude.

To a certain extent all of these objections have been met in the later discussion by Auwers, to which reference will presently be made. The substantial agreement of these two systems, independently determined, furnishes satisfactory evidence that we have at last obtained a foundation system with which it is safe to make comparisons—from which we may draw conclusions with comparative safety. When the catalogues which were formed between 1825 and 1865 are compared with Newcomb's fundamental system, through the medium of these thirty-two stars, the following facts are revealed:—

a. The only catalogues in which there is freedom from both accidental and periodic errors are Argelander's Åbo catalogue for 1830 and the Pulkowa catalogue for 1845. One is reminded in this connection of the remark of Pond, that "we can hardly obtain a better test of our power of predicting the future positions of stars than by trying by the same formula how accurately we can interpolate for the past. In a variety of papers which I have submitted to the Royal Society I have endeavoured to show that with us the experiment *entirely* fails."

b. During this interval the constant differences between the earlier catalogues and Newcomb's system vary between +0·17s. for Pond, 1820, and $-$0·19s. for Pond, 1830; and for later catalogues between +0·07s. for Cambridge, 1860, and +0·02s. for Greenwich, 1860.

c. All the right ascensions determined at English observatories, and especially those which depend upon the positions published by the British *Nautical Almanac*, are too large in the region of five hours, and too small in the region of eighteen hours. The general tendency of the constant part of the deviation from Newcomb's system is to neutralise the periodic errors in the region of five hours, and to augment them in the region of eighteen hours, where, in the case of a few catalogues, the error becomes as great as 0·10s.,—a quantity which can be readily

detected from the observations of two or three evenings with an indifferent instrument, if it relates to a single star.

The right ascensions determined at French observatories exhibit systematic errors which follow nearly the same law as those which characterise English observations.

Distinctively German observations are nearly free from systematic errors. As far as they exist at all, their tendency is to neutralise the errors inherent in distinctively English and French observations.

d. In the case of several catalogues residual errors of considerable magnitude remain after the systematic errors depending upon the right ascensions have been allowed for. These errors are found to be functions of the declination of the stars observed, and without doubt have some connection with the form of the pivots of the instrument with which the observations were made. This statement holds true, especially with respect to the observations at Paris, Melbourne, and Brussels, between 1858 and 1871; and to the Washington observations between 1858 and 1861.

e. The systematic errors which exist in observations previous to 1865 follow the same law and have nearly the same magnitude as the errors of the same class which are inherent in the national ephemerides of the country in which they were made.

The British *Nautical Almanac* and the *Connaissance des Temps* are largely responsible for the perpetuation of this class of errors. For a few years before and after 1860 the ephemerides of the *Nautical Almanac* were based upon the observations of Pond, which contain large periodic errors. It is found that the errors of this system have been transferred without sensible diminution to every catalogue in which the observations depend upon *Nautical Almanac* clock stars. At English observatories it has been the custom to correct the positions of the fundamental stars by the observations of each successive year; but this has produced no sensible effect on the diminution of the periodic errors, which belong to the fundamental system. The periodic errors of the *American Ephemeris* follow nearly the same law as the errors of the *Nautical Almanac*, but their magnitude is somewhat reduced. The error of equinox is also less.

Wolfer's *Tab. Reg.*, upon which the *Berliner Jahrbuch* is based, has no well-defined systematic errors, and the correction for equinox is nearly the same in amount as in the *American Ephemeris*, but with the opposite sign. The accidental errors seem to be rather larger than in the system of the *American Ephemeris*.

f. A general estimate may be formed of the relative magnitudes of the errors of secondary catalogues by comparing the average error for each star of the primary catalogue. The numbers given below represent the average deviation for each star, expressed in hundredths of seconds, after the various catalogues have been reduced to a common equinox:—

		Average error for each star.
Argelander	1830	1·1
Pulkowa	1845	1·1
Greenwich	1845	2·0
Greenwich	1860	2·0
D'Agelet (Gould)	1783	2 2
Cape of Good Hope (Henderson)	1833	2·2
Greenwich	1850	2·2
Greenwich	1871	2·2
Paris	1867	2·4
Washington	1846–52	2·5
Struve	1830	2·5
Cape of Good Hope	1856	2·8
Radcliffe	1860	3·1
Greenwich	1840	3·1
Bessel	1825	3·2
Pond	1830	3·7
Gillis	1840	3·8
Madras (Taylor)	1830	3·9
Cape of Good Hope (Fallows)	1830	3·9
Radcliffe	1845	4·5
Armagh	1840	5·0
Piazzi	1800	5·3
Bessel's Bradley	1755	7·9
Lalande	1800	13·2
Lacaille	1750	24·9

It is obvious from these relations that previous to about 1825 the magnitude of the accidental errors of observation, combined with the errors of reduction, prevent any definite conclusions with respect to the periodic errors inherent in these early observations. It is probable, also, that early observations of stars of the eighth and ninth magnitudes are subject to a class of errors peculiar to themselves, the nature of which is now well nigh impossible to determine.

The systematic errors in declination which belong to the various secondary catalogues named are even more marked than those in right ascension. The experience of Pond in 1833 is the experience of every astronomer who has attempted to compare observations of the same star made at different times, under different circumstances, with different instruments, and by different observers. He says: "With all these precautions, we do not find by comparing the present observations with those of Bradley made eighty years ago under the same roof, and computed by the same table of refractions, that we can obtain by interpolation any intermediate catalogue which shall agree with the observations within the probable limits of error."

We owe to the investigations of Auwers (*Astron. Nachr.*, Nos. 1532-1536), the first definite system of declinations which is measurably absolute in its character. Yet the deviations of this system from that derived by the same author, but from much additional data in Publication xiv. of the *Gesellschaft*, is no less than 1·2s. The present difference outstanding between the Pulkowa and the Greenwich systems at 10° south declination is 1·7s.

Within the past five years the labours of Auwers, of Safford, of Boss, and of Newcomb, have resulted in the establishment of a mean system of declinations from which accidental errors may be considered to be eliminated in the case of a large number of stars; but the different systems still differ systematically *inter se* by quantities which are considerably greater than the probable error of any single position.

When the discussion of the question of a uniform determination of all the stars in the northern heavens to the ninth magnitude was taken up by the *Gesellschaft* at its session in Leipzig in 1865, Argelander, who was then president of the Society, appears to have been the only astronomer who had a clear apprehension of the difficulties of the problem. He alone had detected the class of errors whose existence subsequent investigations have definitely established. He alone had found a well-considered plan by which these errors might be eliminated, as far as possible, from future observations.

Argelander, however, always claimed for Bessel the first definite proposal of the proposition under consideration (see *Astron. Nachr.*, i. 257). It was in pursuance of this plan that the zones between −15° and +15° in declination were observed. These zones were to form the groundwork of the Berlin charts; and Argelander, in the execution of the Bonner *Durchmusterung*, simply carried out the second part of Bessel's recommendation.

With the exception of the observations of Cooper at Markree Observatory, and the charts of Chacornac, these two great works —the second being a continuation of the first, under a better and more feasible plan—are the only ones in existence which give us any knowledge of the general structure of the stellar system.

The observations of stars to the ninth magnitude, found in the catalogues of Bessel, Lalande, and Piazzi, form the groundwork of these charts. The coordinates in right ascension and declination of the stars found in these authorities were first reduced to the epoch 1800; the resulting right ascension being given to seconds of time, and the declination to tenths of minutes of arc. With these places as points of reference, all other stars were filled in, down to the ninth magnitude, by observations with equatorial instruments. The work was divided into zones of one hour each. Bremiker undertook five zones; Argelander and Schmidt, two; Wolfers, three; and Harding, two. The remaining zones were undertaken by different astronomers in widely separated localities.

The work seems to have been performed with somewhat unequal thoroughness, some zones containing nearly all the stars to the ninth magnitude, while in others a large number of stars having this limit in magnitude are wanting.

The *Durchmusterung*, undertaken by Argelander at Bonn, was a far more serious and well-considered undertaking. This unequalled work consists in the approximate determination of the coordinates of 324,198 stars situated between −2° and +90° declination. It includes stars to the 9·5 magnitude, the coordinates being given to tenths of seconds of time, and the declinations to tenths of minutes of arc.

The first definite proposal of this work undertaken by the *Gesellschaft*, however, appears to have been made by Bruhns. In

the course of a report upon the operations of the Leipzig Observatory, he stated that in his view the time had come for undertaking a uniform system of determinations of the places of stars to the ninth magnitude in the northern hemisphere by means of meridian circles; but he proposed at the same time that the positions of stars fainter than the ninth magnitude should be determined by means of differential observations with equatorial instruments. After explaining certain plans and arrangements relating particularly to his own observatory, he introduced the following resolution:—

"The *Astronomische Gesellschaft* regards it as needful that all the stars to the ninth magnitude occurring in the *Durchmusterung* should be observed with meridian circles, and commissions the Council to arrange for the execution of the work."

This proposal occasioned a long and somewhat animated discussion, in which Argelander, Hirsch, Bruhns, Förster, Schönfeld, and Struve took part.

Argelander declared himself surprised at this proposal, which called for the rapid realisation of a plan of organisation which he had been considering for years with the greatest care, the difficulties of which he had maturely considered, and the execution of which still demanded the most careful deliberation and preparation. One of the necessary preliminary steps was a plan which he had already prepared, published, and presented to the Society in an informal way, which provided for contemporaneous and corresponding observations of the brighter stars. As president of the Society he felt unequal to undertaking the charge which the acceptance of the resolution proposed would involve, as this procedure seemed to him premature without previous preparation. He would admit, however, that every call to action of this kind tended to stimulate enthusiasm, and should therefore be encouraged, but he felt obliged to ask the Society not to require from him the immediate execution of the plan, but to intrust the serious consideration of it and the preparation for it to his zealous friends in the Council.

Upon the motion of Struve, the Society, by a rising vote, expressed its confidence in the assurance of the president that he would bring forward his plan at the proper time, as soon as the means for its execution could be assured.

At the meeting held at Bonn in 1867 Argelander again brought up the subject in a communication which appears to have been an exhaustive discussion of the whole problem. This paper is not printed in the *Proceedings of the Gesellschaft*, but at its conclusion a committee was appointed to take definite action with respect to the recommendations which it contained. The committee reported at the same session, and their report, which is published in the place of the paper presented by Argelander is probably identical in substance with it. The plan proposed and adopted was finally published in the form of a programme, in which the details of the work are arranged with considerable minuteness. As this programme has been widely distributed, it seems unnecessary to give anything more than a general abstract of it. Since it differs in a few minor points from the first report of the committee at the Bonn meeting, the essential features of this report will be given instead of an abstract of the programme itself.

They are as follows:—

a. The limits in declination of the proposed series of observations are $-2°$ and $+80°$. The first limit was chosen on account of the lack of suitable fundamental stars south of the equator. It is probable, also, that Argelander had a suspicion of the fact, since proven, that the uncertainty with respect to the systematic errors of southern stars is, of necessity, considerably greater than for northern stars, and that on this account it would be better to defer this part of the work until further investigations in this direction could be made.

The limit $+80°$ was chosen because the repetition of Carrington's observations between 81° and 90° was considered superfluous, and Hamburg had already undertaken the extension of Carrington's observations from 81° to 80°.

b. Within these limits, all stars in the *Durchmusterung* to the ninth magnitude, and, in addition, all stars which have been more exactly observed by Lalande, by Bessel at Königsberg, and by Argelander at Bonn, are to be observed.

c. The observations are to be differential. The clock errors are not to be found from the fundamental stars usually chosen for this purpose, and the equator point corrections are not to be derived from observations at upper and lower culminations, but these elements are to be derived from a series of 500 or 600 stars, distributed as uniformly as possible over the northern heavens. The exact coordinates of these stars are to be determined at Pulkowa, thus securing the unity necessary in order to connect in one system the observations of different zones.

d Every star is to be observed twice. If the two observations differ by a quantity greater than ought to be expected, a third observation will be necessary.

e. In order to facilitate the work it will be desirable to use only three or four transit threads and only one or two microscopes. In order to facilitate the reductions to apparent place the working-list of stars should be comprised within narrow limits.

f. Before the commencement and after the close of each zone, two or three fundamental stars are to be observed upon the same threads and with the same microscopes as were used in the zone observations. When the seeing is not good, and when for any other cause it seems desirable, one or more fundamental stars may be observed in the course of the zone. The number and selection of the stars will depend upon the character of the instrument employed. If it remains steady for several hours and has no strongly marked flexure or division errors, or if these errors have been sharply determined, the fundamental stars may be situated ten degrees or fifteen degrees away from the zone limits. However, there must remain many things for which no general rule can be given, and which must be left to the judgment of the observer, aided by an accurate knowledge of his instrument.

g. With a Repsold or a Martin instrument one microscope will be sufficient, if its position with respect to the whole four can be determined. It will be sufficient if the change in position during the observations can be interpolated to 0·2s.

h. It will be desirable to divide beforehand the zones into such time intervals that the observations can be easily made.

i. Zones exceeding one and a half or at the most two hours are not advisable, first, because the zero points will be too far apart, and, second, because a longer duration will involve too much fatigue physically and mentally.

At the conclusion of this report all the astronomers present who were willing to take part in this work were requested to communicate with the Council, stating the regions of the heavens which they preferred to select for observation.

At this meeting, Berlin, Bonn, Helsingfors, Leipzig, and Mannheim signified their intention to share in the work. Leyden also expressed its intention of taking part as soon as the work already undertaken should be completed.

When the stars to be observed had been selected from the *Durchmusterung*, it was found that the number would not vary much from 100,000, requiring rather more than 200,000 observations. Preparations for the work of observation were immediately commenced, and, by the time of the next report in 1869, considerable progress had been made.

In the report for this year the provisional places of a catalogue of 539 fundamental stars were published. This catalogue is composed of two parts. The list of *Haupt-sterne* consists of 336 stars to the fourth magnitude, observed at Pulkowa by Wagner with the large transit instrument, and by Gyldén with the Ertel vertical circle. The list of *zu za t-sterne* consists of 203 stars fainter than the fourth magnitude. As the details of the work in the formation of the provisional places of the stars of this list are not given in the report, it is not quite clear upon what authority they rest. The work assigned to the Pulkowa observatory by the Zone Commission was the exact determination of the places of the stars of this list. The observations were undertaken by Gromadski with the Repsold meridian circle. In accordance with the plan adopted each star was observed eight times—four times in each position of the instrument. The observations were differential with respect to the *Haupt-sterne*.

The results were published by Struve in 1876, and the places there given were used in the first reduction of the Harvard College observations for 1874-75, and perhaps in some other cases.

About this time a change seems to have been made in the original plan with respect to the formation of the final catalogue of fundamental stars, of which I have been unable to find a clear account. The original intention was to make the positions depend entirely upon the observations at Pulkowa. The Zone Commission established by the *Gesellschaft*, however, committed the formation of this catalogue to Auwers; and it is to him that we owe the most complete and the most perfect catalogue of fundamental stars yet published. The Pulkowa system for 1865 was adopted as the basis; but, in order to obtain greater

freedom from accidental errors for individual stars, the final catalogue was obtained by combining with the Pulkowa series the Greenwich observations from 1836 to 1876, the Harvard College observations for 1871–72, the Leipzig observations in declination only, between 1865 and 1870, and the Leyden observations in declination between 1864 and 1870. Before this combination was made, however, these observations were all reduced to the Pulkowa system.

The following observatories have taken part in the zone observations:—

Observatories.	Limits of zones in declination.	Observatories.	Limits of zones in declination.
Nicolaieff ...	-2° to $+1^\circ$	Lund	$+35^\circ$ to $+40^\circ$
Albany	$+1$,, $+5$	Bonn	$+40$,, $+50$
Leipzig	$+4$,, $+10$	Harvard College	$+50$,, $+55$
Leipzig	$+10$,, $+15$	Helsingfors ...	$+55$,, $+60$
Berlin	$+15$,, $+25$	Christiania ...	$+65$,, $+70$
Cambridge (Eng.)	$+25$,, $+30$	Dorpat	$+70$,, $+75$
Leyden	$+30$,, $+35$	Kasan	$+75$,, $+80$

The zone between -2° and $+1^\circ$ was originally undertaken at Palermo, that between $+1^\circ$ and $+4^\circ$ at Neuchâtel, that between $+4^\circ$ and $+10^\circ$ at Mannheim, and that between $+35^\circ$ and $+40^\circ$ at Chicago.

In the latter case the great fire at Chicago crippled the resources of the observatory to such an extent that Safford was compelled to relinquish the work, which was at that time quite far advanced.

Attention was called at an early date to the importance of continuing the survey of the northern heavens beyond the southern limit fixed by Argelander. The preparation necessary for the execution of this work consisted in the extension of the *Durchmusterung* to the tropic of Capricorn. This was undertaken by Schönfeld at Leipzig.

In the report to the *Gesellschaft* at the meeting held at Stockholm in 1877, he has given an account of this work, in which he stated that it was sufficiently near completion to invite the consideration of the question of the meridian circle determinations of the places of stars to the ninth magnitude. The lack of southern fundamental stars whose positions were well determined was still a hindrance to the immediate commencement of the work. Relatively more stars of this class are required than in the northern observations, in order to eliminate the inequalities due to refraction. Schönfeld stated that, while the burden of the determination of the places of these southern fundamental stars must rest mainly upon southern observations, it seemed necessary to connect them with the Pulkowa system by a connecting link (*Mittelnlied*), through observations at some observatory well situated for this purpose. At this meeting Sande Bakhuysen, of Leyden, gave notice of intention to take part in this work. Gyldén urged the importance of securing the cooperation of Melbourne, and Peters suggested the advantage of securing Washington as an additional "mean term" (V.J.S. 1877, p. 265).

The next reference to this work is contained in the *Gesellschaft* for 1881 (V.J.S. xv. p. 270). A list of 303 southern stars is here given whose exact places were at that time being determined at Leyden and at the Cape of Good Hope. This list was selected by Schönfeld and Sande Bakhuysen, in a way to meet the requirements referred to in previous discussions.

A final catalogue of eighty-three southern fundamental stars by Auwers appears in this number of the *Gesellschaft.* The places depend upon the same authorities as for the northern stars, with the addition of the Cape of Good Hope catalogue for 1860, Williamstown, Melbourne for 1870, and Harvard College (Safford) for 1864. For stars not observed at Pulkowa, the general catalogue of Yarnall (1858-1861), and the Washington observations, with the new meridian circle between 1872 and 1875, were employed. As in the case of the northern stars, these observations are all reduced to the Pulkowa system for 1865. It is understood that the coordinates of the list of 303 stars are to depend upon this extension of the general system of Publication xiv. to the limits required by the southern *Durchmusterung* of Schönfeld.

It would be surprising if all the conditions of success were fulfilled in the first execution of a work having the magnitude and involving the difficulties of the scheme of observations undertaken under the auspices of the *Gesellschaft.* The extent of the discordances which are to be expected between the results obtained by different observers can only be ascertained when the observations by which the different zones are to be connected have been reduced. Each observer extended the working list of his own zone 10′ north and south; and it is expected that a sufficient number of observations of this kind have been made to determine the systematic relations existing between the coordinates of each zone with those of its neighbour.

It is probable, however, that the experience of Gill will be repeated on a larger scale. In 1871 he solicited the cooperation of astronomers in the determination of the coordinates of twenty-eight stars, which he desired to employ in the reduction of his heliometer observations of the planet Mars for the purpose of obtaining the solar parallax. The results obtained at twelve observatories of the first class are published in vol. xxxix. p. 99, of the *Monthly Notices of the Royal Astronomical Society.* Notwithstanding the fact that the final values obtained at each observatory depend upon several observations, the average difference between the least and the greatest results, obtained by different observers for each star, is 0·24s. in right ascension, and 2·3″ in declination. In four cases the difference in right ascension exceeds 30s., and in four cases the difference in declination exceeds 3·0″.

Even after the results are reduced to a homogeneous system, the following outstanding deviations from a mean system are found:—

Authority.	$\Delta\alpha$ s.	$\Delta\delta$ ″	Authority.	$\Delta\alpha$ s.	$\Delta\delta$ ″
Königsberg	+·005	−0·71	Leyden ...	−·053	−0·19
Melbourne	+·026	−0·49	Paris	+·055	+0·01
Pulkowa ...	+·005	+0·36	Washington	−·120	+0·78
Leipzig ...	+·049	+0·40	Harvard Coll.	−·072	+0·09
Greenwich ...	+·009	−0·56	Cordova ...	−·032	−0·20
Berlin ...	+·044	+0·67	Oxford ...	+·076	+0·21

The observations of a second list of twelve stars, one-half of the number being comparatively bright, and the remaining half faint, showed no marked improvement, either with respect to the magnitude of errors which could be classed as accidental, or in regard to the systematic deviations from a mean system.

This discussion revealed one source of discordance which will doubtless affect the zone observations: viz. the difference between the right ascensions determined by the eye-and-ear method and those determined with the aid of the chronograph.

The programme of the *Gesellschaft* makes no provision for the elimination of errors which depend upon the magnitude of the stars observed; but special observations have been undertaken at several observatories for the purpose of defining the relation between the results for stars of different magnitudes. At Harvard College Observatory, the direct effect of a reduction of the magnitude has been ascertained by reducing the aperture of the telescope by means of diaphragms. Besides this, the observations have been arranged in such a manner that an error depending upon the magnitude can be derived from an investigation of the observations upon two successive nights.

At Leyden, at Albany, and perhaps at other observatories, the effect of magnitude has been determined by observations through wire gauze. But notwithstanding all the precautions which have been taken in the observations, and which may be taken in the reductions, it will undoubtedly be found that the final results obtained will involve errors which cannot be entirely eliminated.

In the experience of the writer two other sources of error have been detected. It has been found that there is a well defined equation between the observations, which is a function of the amount, and the character of the illumination of the field of the telescope. It has also been found that observations made under very unfavourable atmospheric conditions differ systematically from those made under favourable conditions. When the seeing was noted as very bad, it is found that the observed right ascensions are about ·08s. too great, and that the observed declinations are about 0·8″ too great.

There are doubtless other sources of error which the discussion of the observations will bring to light. The effect of the discovery of these and other errors will probably be to hasten the repetition of the zone observations under a more perfect scheme, framed in such a manner as to cover all the deficiencies which experience has revealed or may yet reveal. One would not probably go far astray in naming the year 1900 as the mean epoch of the new survey. If the observations are again repeated in 1950, sufficient data will then have been accumulated for at least an approximate determination of the laws of sidereal motion.

What is the present state of our knowledge upon this subject? It can be safely said that it is very limited. First of all it cannot be affirmed that there is a sidereal system in the sense in which we speak of the solar system. In the case of the solar system we have a central sun about which the planets and their satellites revolve in obedience to laws which are satisfied by the hypothesis of universal gravitation. Do the same laws pervade the interstellar spaces? Is the law of gravitation indeed universal? What physical connection exists between the solar system and the unnumbered and innumerable stars which form the galaxy of the heavens? Do these stars form a system which has its own laws of relative rest and motion, or is the solar system a part of the stupendous whole? Does the solar system receive its laws from the sidereal system, or has Kepler indeed pierced the depths of the universe in the discovery of the laws which gave him immortality? Are we to take the alternative stated by Ball,—either that our sidereal system is not an entirely isolated object, or its bodies must be vastly more numerous or more massive than even our most liberal intepretation of observations would seem to warrant? Are we to conclude, for example, that stars like 1830 Groombridge and α Centauri, "after having travelled from an infinitely great distance on one side of the heavens, are now passing through our system for the first and only time, and that after leaving our system they will retreat again into the depths of space to a distance which, for anything we can tell, may be practically regarded as infinite?" Can we assert with Newcomb, that in all probability the stars do not form a stable system in the sense in which we say that the solar system is stable,—that the stars of this system do not revolve around definite attractive centres? Admitting that the solar system is moving through space, can we at the present moment even determine whether that motion is rectilinear, or curved, to say nothing of the laws which govern that motion. How much of truth is there in the conjectures of Wright, Kant, Lambert, and Mitchel, or even in the more serious conclusions of Mädler that the Alcyone of the Pleiades is the central sun about which the solar system revolves?

These are questions which, if solved at all, must be solved by a critical study of observations of precision accumulated at widely separated epochs of time. The first step in the solution has been taken in the systematic survey of the northern heavens undertaken by the *Gesellschaft*, and in the survey of the southern heavens at Cordova by Dr. Gould. The year 1875 is the epoch about which are grouped the data which, combined with similar data for an epoch not earlier than 1950, will go far towards clearing up the doubts which now rest upon the question of the direction and the amount of the solar motion in space; and it cannot be doubted that our knowledge of the laws which connect the sidereal with the solar system will be largely increased through this investigation. The basis of this knowledge must be the observed proper motions of a selected list of stars, so exactly determined that the residual mean error shall not affect the results derived; or, failing in this, of groups of stars symmetrically distributed over the visible heavens, sufficient in number to effect an elimination of the accidental errors of observation without disturbing the equilibrium of the general system.

For an investigation of this kind, a complete system of zone observations, at widely separated intervals, will afford the necessary data, if the following conditions are fulfilled.

First, the proper motions must be derived by a method which does not involve an exact knowledge of the constants of precession. In every investigation with which I am acquainted the derived proper motions are functions of this element.

Second, the general system of proper motions derived must be free from systematic errors. Errors of this class may be introduced either through the periodic errors inherent in the system of fundamental stars employed in the reduction of the zone observations, or in a change in the constants of precession. It is in this respect that the utmost precaution will be required. If from any cause errors of even small magnitude are introduced into the general system of proper motion at any point, the effect of these errors upon the values of the coordinates at any future epoch will be directly proportional to the interval elapsed. We can, therefore, compute the exact amount of the accumulated error for any given time.

When this test is applied to the fundamental stellar systems independently determined by Auwers, Safford, Boss, and Newcomb, we find the following deviations *inter se* at the end of a century:—

	Maximum mean deviation in a century. $\Delta\alpha$	$\Delta\delta$		Maximum systematic deviation in a century.	
Auwers *minus* Safford ...	−0·22s.	+0·2″	...	0·23s.	1·1′
Auwers *minus* Boss ...	—	+0·8	...	—	2·1
Auwers *minus* Newcomb	−0·09	+0·8	...	0·06	2·2

It is the common impression that both the direction and the amount of the motion of the solar system in space are now well established. The conclusions of Struve upon this point are stated in such explicit language that it is not surprising that this impression exists. He says, "The motion of the solar system in space is directed to a point in the celestial sphere situated on the right line which joins the two stars measured from π and ω Herculis. The velocity of this motion is such that the sun, with the whole *cortège* of bodies depending on him, advances annually in the direction indicated, through a space equal to 154,000,000 miles.

It must be admitted that there is a general agreement in the assignment by different investigators of the coordinates of the solar apex. This will be seen from the following tabular values:—

Authorities.	Right Ascension.		Declination.
	°		°
Herschel, 1783	257 00	...	+25 00
Prevost, 1783	230 00	...	+25 00
Klugel, 1789	260 00	...	+27 00
Herschel, 1805	245 52	...	+49 38
Argelander, 1837	257 49	...	+28 50
Lundahl, 1837	252 24	...	+14 26
Struve, 1837	261 22	...	+37 36
Galloway, 1837	260 01	...	+34 23
Mädler, 1837	261 38	...	+39 54
Airy, 1837	256 54	...	+34 29
	261 29	...	+26 44
Dunkin	261 14	...	+32 55
	263 44	...	+25 00

In estimating the value which should be attached to these results, several considerations must be taken into account.

(*a*) All of the results except those of Galloway depend practically upon the same authorities at one epoch, viz. upon Bradley.

(*b*) The deviations *inter se* probably result in a large measure from the systematic errors inherent in one or both of the fundamental systems from which the proper motions were derived. For example, Lundahl employed Pond as one of his authorities, and it is in Pond's catalogue that the most decided periodic errors exist.

(*c*) Biot in 1812, Bessel in 1818, and Airy in 1860, reached the conclusion that the *certainty* of the movement of the solar system towards a given point in the heavens could not be affirmed.

(*d*) The problem is indirect. In the case of a member of the solar system, exact data will determine the exact position in orbit at a given time; but here we have neither exact data nor can we employ trigonometrical methods in the solution. We simply find that the observed proper motions are probably somewhat better reconciled under the hypothesis of an assumed position of the apex of the solar motion. The method of investigation employed by Safford, who has of late years given much attention to this subject, consists in assuming a system of coordinates for the pole of the solar motion, from which is determined the direction each star would have if its own proper motions were zero. Comparing this direction with the observed direction as indicated by the observed proper motion, equations of condition are formed from which a correction is found to the assumed position of the apex, by the methods of least squares.

It must always be kept in mind that the quantities with which we must deal in this investigation are exceedingly minute, and that the accidental errors of observation are at any time liable to lead to illusory results. The weak link in the chain of Mädler's reasoning is to be found here. I think we can assume 0·20″ as the limit of precision in the absolute determination of the coordinates of any star, however great the number of observations upon which it depends. Beyond this limit it is impossible to go, in the present state of instrumental astronomy.

It is safe to say that there is not a single star in the heavens whose coordinates are known with certainty within this limit. Do not misunderstand me. Doubtless there are many stars in which

the error will at some future time be found to fall within this limit. The law of probabilities requires this, if the maximum limit falls within 1″. But who is prepared to select a particular star and say that the absolute position of this star in space cannot be more than 0·2″ in error?

e. At present an arbitrary hypothesis is necessary in the discussion of the problem. Airy assumed that the relative distances of the stars are proportional to their magnitudes; and he found slightly different results according to different modes of treatment. Safford assumed that the distances are, at least approximately, in inverse proportion to the magnitude of the proper motions. The general result of his investigations up to this point is that there is some hope of using the solar motion as a base to advance our knowledge of stellar distances. Later investigations have been made by De Ball, but the details have not yet come to hand. It is understood, however, that his results coincide in a general way with those previously obtained.

It is clear from this brief review that we have here a field of investigation worthy of the highest powers of the astronomer. The first step has been taken in the survey of the heavens carried on under the auspices of the *Gesellschaft*. It remains for the astronomers of the present generation to solve the difficulties which now environ the problem, and prepare the way for a more perfect scheme of observation in the next century.

INDIAN METEOROLOGY[1]

III.

THE next paper we shall notice is No. IX., by Fred. Chambers, on "The Winds of Kurrachee." The station dealt with is not only a representative one of the Arabian sea current, but is remarkable for exhibiting the highest average monthly wind velocity of any place in India. The observations used were furnished by a Beckley's anemograph for 1873, 1874, and 1875.[2]

In discussing the annual variation, Mr. Chambers adopts a plan which has been followed out with much success by his brother in his great work on the meteorology of the Bombay Presidency, viz. its separation into *normal* and *abnormal* north and east components.

It is thence found that the former are closely related to the corresponding barometric variations, and represent that part of the grand monsoon system which affects Kurrachee, while the latter are found to be connected with a system of local convection currents, due to (relatively) local temperature variations. These latter, though subordinate to the former in point of magnitude, are still sufficiently large to mask the true nature of the regular monsoon currents which obey the barometric law. This is more especially the case in Bengal, where, as it appears both from evidence furnished in this paper and elsewhere, the activity of the monsoon currents is far less than on the west coast of India, while the absolute efficiency of the *local* variations is about the same.[3]

Another important result deduced, is that the causes which produce the abnormal variations in the wind and pressure components, are similar to those which produce the annual variations. Thus, when the barometer rises abnormally a tenth of an inch, it is accompanied by an abnormal wind of 4·4 miles per hour from N. 55° E., while a similar rise in the barometer from summer to winter gives rise to a wind of 4·7 miles per hour from N. 57° E.

This principle, which, though *a priori* probable, has not hitherto been supported by direct evidence, is without doubt destined to play an important part in the meteorology of the future, and to form one of the few channels by which we may hope to arrive at a correct knowledge of the effects of the suspected intrinsic variation of solar radiation on terrestrial meteorology. Thus Mr. Chambers says: "If the sun's heat is itself subject to fluctuations, either periodical or irregular, corresponding meteorological effects similar to those which are produced by the sun's change of position must result;" and he adds: "The relation at Kurrachee ppears to be one of the kind that would be anticipated on the supposition of the sun's heat being variable, and in itself affords a reason for suspecting, if it does not tend to prove, such variability."

In discussing the diurnal variations, Mr. Chambers divides the winds into two great classes, *convection* or ordinary currents, in which the air moves from relatively cool to relatively warm regions, and *anti-convection* currents, or "winds of elastic expansion" as Blanford calls them, which blow outwards from regions of high temperature. Each of these classes is again divisible into two sub-classes, (1) general and (2) local.[1]

If each of these systems is possible, as Mr. Chambers infers, the resultant variation is evidently a very complex one, and the main difficulty in discussing it, evidently consists in being able to adequately separate each component in turn from the rest. For this purpose Mr. Chambers employs Bessel's formula, and though he admits that the components derived by this method, do not necessarily represent physically distinct variations, its use in this case, as well as in others throughout this work, is attended with such favourable results, as to constitute a plea in favour of its more general adoption by English meteorologists.

To follow all the details of the investigation would be beyond our scope. It may therefore be briefly noted that the greater part of the variation of the north component, is due to the alternate land and sea breeze (convection currents), while a portion at any rate of the variation of the east component, is due to local anti-convection currents which prevail only in the drier months. Further, the direction of the local anti-convection currents varies with the varying position of the centre of maximum temperature range in the peninsula, while that of the coast convection currents is nearly constant.

By an ingenious plan for eliminating the variations due to coast convection currents, and by choosing the months so as to reduce the local anti-convection currents to a minimum, the existence is further proved of a system of general anti-convection currents, which, it may be remarked, were first noticed by Mr. Laughton in 1871, consisting of a double diurnal oscillation of the east component, which in the case of Kurrachee reaches its maxima at 10 a.m. and 9 p.m. and its minima at 4 p.m. and 2 a.m. respectively. These general anti-convection currents have been likewise proved by Mr. Chambers to exist at Calcutta, Belgaum, Bermuda, and Falmouth, *i.e.* in places where the ordinary convection currents differ completely both in character and intensity.

A comparison of the rainfall with the wind at the end of this paper leads to a conclusion similar to that drawn by Mr. Blanford, viz., that rain seldom falls as long as the summer monsoon continues to blow steadily, and Mr. Chambers hence infers, that a strong, damp wind from the seaward, is not the only condition required to produce rain. If this rule is only meant to apply to the place where the wind prevails, it is doubtless correct; but it seems open to misinterpretation if taken in a more general sense, since the laws of cyclonic systems and experience, both tell us that the reason why there is little rain on the coast when the sea wind is blowing strongly, is because the area of lowest pressure towards which the wind is spirally blowing is situated in the interior of the country, and that when there is *least* rain on the coast there is probably *most* inland.

Paper X. "Some Results of the Meteorological Observations taken at Allahabad during the Ten Years 1870–79," by S. A. Hill.—This paper, which represents the most complete discussion of the climatic elements at a single station in the interior of India that has ever been published, contains much that is valuable and highly suggestive to the physical meteorologist. To the climatologist it is especially interesting, owing to the inland as well as tropical position of the station. In May and June, Allahabad is one of the hottest places in India, the maximum temperature in the shade often rising above 115° Fahr., while in that terribly hot year, 1878, the temperature actually rose up to 119°·8 on June 19.

Nearly all the elements are discussed by the aid of Bessel's formula, and as it is a paper which cannot readily be reviewed in detail, we propose merely noticing one or two of the most salient conclusions deduced by the author.

One remarkable feature that comes out from the discussion of the diurnal barometric oscillation, is its "continental" character. Like Yarkand and other typically continental stations, the fall of the night tide is very small, and the ratio of the amplitude of the semi-diurnal to the diurnal component, is not only smaller than

[1] Continued from p. 430.

[2] The small elevation of the anemograph (only 15·6 feet above the ground) is open to some objection, but this is a good deal compensated for by its unusually free exposure.

[3] The resultant ranges of the wind variations obeying the barometric law are as follows:—

Kurrachee	26·6
Bombay	20·5
Calcutta	6·2

[1] These latter are dealt with in detail in those papers of Mr. Chambers's which have already been alluded to.